

SCIENCE.

EDITORIAL COMMITTEE: S. NEWCOMB, Mathematics; R. S. WOODWARD, Mechanics; E. C. PICKERING, Astronomy; T. C. MENDENHALL, Physics; R. H. THURSTON, Engineering; IRA REMSEN, Chemistry; JOSEPH LE CONTE, Geology; W. M. DAVIS, Physiography; O. C. MARSH, Paleontology; W. K. BROOKS, Invertebrate Zoölogy; C. HART MERRIAM, Vertebrate Zoölogy; N. L. BRITTON, Botany; HENRY F. OSBORN, General Biology; H. P. BOWDITCH, Physiology; J. S. BILLINGS, Hygiene; J. McKEEN CATTELL, Psychology; DANIEL G. BRINTON, J. W. POWELL, Anthropology.

FRIDAY, FEBRUARY 15, 1895.

CONTENTS:

<i>The Influence of Certain Agents in Destroying the Vitality of the Typhoid and of the Colon Bacillus:</i> JOHN S. BILLINGS and ADELAIDE WARD PECKHAM	169
<i>Current Notes on Physiography (I.):</i> W. M. DAVIS	174
<i>The Needs of Meteorology:</i> CLEVELAND ABBE	181
<i>Correspondence:—</i>	182
<i>A Card Catalogue of Scientific Literature:</i> H. P. BOWDITCH.	186
<i>Scientific Literature:—</i>	186
<i>Organic Coloring Matters:</i> IRA REMSEN. <i>Thompson's Electricity and Magnetism:</i> T. C. M. <i>The Birds of Eastern Pennsylvania; Birds in the Museum of Natural History, New York City:</i> C. HART MERRIAM. <i>Russell's Dairy Bacteriology:</i> H. W. C. <i>Botanical:</i> ALBERT SCHNEIDER.	190
<i>Notes and News:—</i>	193
<i>The Thompson Prize; Entomology; Cooling of Hospitals; Pithecanthropus Erectus.</i>	196
<i>Societies and Academies:—</i>	196
<i>New York Academy of Sciences, Section of Geology and Mineralogy.</i>	196
<i>Scientific Journals</i>	196
<i>New Books</i>	196

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison on Hudson, N. Y. Subscriptions (five dollars annually) and advertisements should be sent to the Publisher of SCIENCE, 41 East 49th St., New York, or Lancaster, Pa.

THE INFLUENCE OF CERTAIN AGENTS IN DESTROYING THE VITALITY OF THE TYPHOID AND OF THE COLON BACILLUS.

DURING the last year a series of researches upon the influence of light, of desiccation, and of the products of certain micro-organisms upon the vitality of some

of the pathogenic bacteria has been carried on in the Laboratory of Hygiene of the University of Pennsylvania, by Dr. Adelaide W. Peckham, in accordance with a general scheme for such investigation prepared by Dr. Weir Mitchell and Dr. Billings, the Director of the Laboratory, and with the aid of a grant from the Bache fund. A portion of the results obtained in this research has been communicated to the National Academy of Sciences at its meetings in April and in October, 1894; but as the volume of the Transactions of the Academy which will contain these papers will not be issued before next year, it has been thought best to publish some account of these experiments without further delay.

That direct sunlight kills or stops the growth of certain bacteria has been known since 1877, when Downes and Blunt presented to the Royal Society a report on "Researches on the effects of light upon bacteria and other organisms."* Since that date a number of papers on this subject have been published, the most important one in relation to the typhoid bacillus being that of Janowski in 1890.† The first series of experiments by Dr. Peckham was made with the *staphylococcus pyogenes aureus*, the object being mainly to determine the best methods of investigation.

* Proc. Roy. Soc. 1877, vol. 26, p. 488.

† Zur Biologie der Typhus Bacillen, Centralbl. Bakteriöl, etc., VIII., 1890, pp. 167, 193, 230, 262.

Photobacteriographs were made by Buchner's method, namely, by placing a square of black paper, or of glass of different colors, upon the bottom of a plate containing inoculated agar-agar during insolation; but although the protected portion was visible after fifteen minutes' insolation and incubation for twenty-four hours, and sharply defined after two hours' insolation and incubation as before, no accurate estimate could be made of the difference in the destructive power of different periods of insolation. Successful photobacteriography requires inoculation of large quantities of bacteria, in order that the colonies may be set so closely together that a ground-glass appearance is produced; in which case counting of the colonies is practically impossible.

For this reason the following method was used for each of the three organisms, the *staphylococcus pyogenes aureus*, the *bacillus coli communis* and the *bacillus typhi abdominalis*.

To obtain an accurate measure of the effects produced by lights of different intensity or of different colors, it is necessary to ensure, as far as possible, that the bacteria to be experimented on shall be uniformly distributed in the culture media. Tubes containing each 10 cc. of bouillon were inoculated with one drop of a bouillon culture and then placed in an incubator for twenty-four hours. A small quantity of sterilized gravel was then added to the culture tube and it was thoroughly shaken, after which 10 cc. of a one-half per cent. salt solution was added and the culture drawn into a Nuttall's dropping apparatus. From this, one-twentieth of 1 cc. of the bouillon culture was dropped into a tube of melted agar-agar, which was slowly and thoroughly agitated, and the contents were then poured into a Petri dish, carefully levelled on a levelling tripod over ice water. In the first method used the Petri dishes

were found to be so uneven on the bottom that the layer of medium under the protective square was often very thick or very thin as compared with that about the circumference of the plate, and, therefore, comparisons made between the centre and the circumference would be in almost every case unreliable. To overcome this difficulty, just one-half of the plate was shaded with black paper or colored glass.

The plates were then exposed to sunlight, bottom upwards, so as to allow the sun to shine as directly as possible on the inoculated agar-agar. At intervals of fifteen minutes a plate was removed and placed in the incubator. The temperature of the plates during insolation was always below 34° C. as shown by a thermometer with a blackened bulb which was placed in the sun and the temperature noted every fifteen minutes. Sunny, still days were utilized for insolation, beginning at 10 A. M. during the months of October, November and December. After insolation, the plates, and also a non-insolated control plate were incubated for twenty-four hours.

The colonies were counted in the following manner: A number 1 eye-piece was divided into fields (as done by Nuttall in counting tubercle bacilli), by introducing a disk of black cardboard which had a square opening divided into four parts by two hairs placed at right angles. This eye-piece and an objective of low power were used in counting. The percentage of germs destroyed by insolation was estimated from the mean of four counts taken on both the insolated and the protected halves of the plate. By this method an accurate statement can be made regarding the difference in protective power given by the different colors, not from simple observation, but by comparison of a definite number of colonies counted.

The following table shows the comparative effect of the blue rays and of complete

shadows on the growth of the organisms experimented on :

Number of minutes exposed.....		Percentages of organism destroyed in the insulated half of the plate as compared with the protected half.							
		15	30	45	60	75	90	105	120
Typhoid	Shaded with { black paper..	17	28	33	34	65	63	90	98
	{ blue glass....	7	14	30	32	24	38	35	52
Colon ...	Shaded with { black paper..	25	15	25	71	83	88	97	99
	{ blue glass....	18	29	32	35	56	59	60	52
Aureus ..	Shaded with { black paper....	...	...	55	...	72	72	80	90
	{ blue glass....	...	38	34	54	51	41	48	50

From this series of experiments the following results were obtained :

Insolation for fifteen minutes destroys to a slight extent each of the three organisms experimented upon. Two hours' insolation destroys 98% of the germs and from three to six hours kills all. The colon bacillus is more easily destroyed by insolation than is the typhoid bacillus. Exposure to diffuse daylight, to gas light, or to the incandescent electric light produces little effect. Red, orange, yellow, and green light produce little effect, during two hours' insolation; while the blue and violet rays kill nearly as rapidly and as certainly as full sunlight. Insolation from six to eight hours lessens the number of colonies under the protective square to a slight extent, for the colors red, orange, yellow and green.

Plates were made in the same manner and exposed to diffused light for periods varying from fifteen minutes to two days. The exposure was made on clear sunny days in the light part of a room. In this experiment the result was negative, the number of colonies on the two sides of the plate being approximately the same.

An ordinary gas-burner and an incandescent light were each used as the source of illumination. The plates were placed bottom-upwards in a dark room near the light used. Illumination for sixteen hours with

gas produced no effect on the growth of the organism as shown by counting of the colonies.

Illumination for four and one-half hours with an incandescent light also gave negative results.

A series of experiments was made with tubes of bouillon inoculated with the different organisms and then enclosed in larger tubes containing fluids of different colors—red, orange, yellow and blue, which were exposed to sunlight with control tubes, one placed in water, and the other in a similar tube covered with black paper. The materials used for making the colored solutions were corallin, chromate and bichromate of potassium, and methylene blue. From these tubes, plates were made, and the number of colonies counted.

It was found that an increase in the number of colonies continued to the eighteenth day, the number being greater in the colon and aureus cultures than in the typhoid. The colonies then began to decrease, and on the fifty-eighth day the plates contained but few colonies. In this experiment, as in the last, plates made from culture tubes placed in blue fluid showed fewer colonies.

Since the presentation of the above results, with details, charts and tables, to the National Academy, in April, 1894, Dr. Dieudonné has published in the *Arbeiten aus dem Kaiserlichen Gesundheitsamte*, a paper on the effects of sunlight on bacteria, in which he reports results substantially the same, and obtained by almost the same methods as those of Dr. Peckham.

Sunlight not only weakens or kills the typhoid and the colon bacillus, but it affects culture media so as to render them less capable of supporting the growth of these organisms. Dr. Peckham found that sterile bouillon insulated from one to ten days and then inoculated with the *bacillus typhi abdominalis* showed no diminution in the number of colonies as compared with a control

plate made from a similar culture not so exposed. Twenty days insolation and then inoculation with the typhoid bacillus showed great decrease in the number of colonies on all the plates; some of them were sterile. Insolation of forty days, and inoculation in the same manner, gave very few colonies for each plate, probably the same as the number of germs introduced, *i. e.*, there had been no development. Bouillon insolated 50—60 days and inoculated gave sterile tubes. This insolated bouillon after inoculation and incubation remained perfectly clear, and plates made after a week of incubation gave no more colonies than those made at the end of twenty-four hours. Its reaction was alkaline, but not intensely so.

Insolated agar-agar.—Of twenty-three tubes of agar-agar insolated twenty days, and then inoculated with the *bacillus typhi abdominalis*, all except one remained sterile, and neither the *bacillus typhi abdominalis* nor the *bacillus coli communis* grew when inoculated in stripes on these plates. Of seven tubes of agar-agar insolated forty days and then inoculated with the bacillus of typhoid, all remained sterile. On four of these plates mould appeared after some days. Of seven tubes of agar-agar insolated forty days and then inoculated and incubated as before, all remained sterile.

Insolated gelatine.—Of ten gelatine tubes insolated forty days and then inoculated with the *bacillus typhi abdominalis*, six remained sterile, two contained a few colonies of *bacillus typhi abdominalis*, and two were contaminated.

The insolated bouillon was then kept in diffuse daylight for forty days and again inoculated with the typhoid bacillus. Within twenty-four hours the tubes of bouillon became turbid and plates made from them showed innumerable colonies.

It is difficult to account for the effect of insolation on culture media. Roux in his

experiments on anthrax found that insolation of bouillon for *two or three hours* rendered it unsuitable for germination of the spores, but if the bacilli were introduced they would thrive. He attributes this alteration to some chemical change which the culture media undergo during the insolation. He found also that if the insolated media were kept in the dark or in diffuse daylight for a time, the original nutritive qualities were restored and germination of spores would take place. Geisler and Janowski observed the bactericidal properties of insolated media, but the latter could find no chemical alteration in such media.

Percy Frankland in his chapter on action of light on micro-organisms* concludes from the results obtained by many investigators 'that the effect is due to a process of oxidation possibly brought about through the agency of ozone or peroxide of hydrogen, or both; that all apparently direct low temperature oxidations require the presence of water. And inasmuch as the bactericidal action of light is unquestionably a case of low temperature oxidation, there is the strongest presumptive evidence, as well as weighty experimental evidence, that moisture, which practically means the possibility of the presence of peroxide of hydrogen or of some similar material, is essential for its manifestation.'† Westbrook ('Some of the effects of sunlight on tetanus cultures, Jour. of Pathol. & Bacteriol. III., Nov. 1894, 71') found that old broth cultures of the tetanus bacillus in an atmosphere of hydrogen were not in the least affected by exposure to sunlight, either in regard to their virulence or their rapidity of growth on reinoculation. When the same culture was sealed up in the presence of air, the

*Micro-organisms in water, p. 390.

†Gelatine, to which were added different amounts of the peroxide of hydrogen, was inoculated with the *bacillus typhi abdominalis* and poured into plates. Those plates in which more than one part of the peroxide to 5000 of gelatine was used, were sterile.

micro-organisms, were not only killed, but the material was completely harmless when inoculated into white mice. It was, however, possible to obtain vigorous and virulent growths from cultures which had been made quite innocuous by the action of the sun. Oxygen was used up in the process. Under ordinary circumstances one might be tempted to explain the effect of sunlight in destroying bacteria by the drying of the organisms exposed to it, especially in the case of those bacteria which do not form spores, but our experiments show that desiccation for months has little effect on the vitality of the typhoid or of the colon bacillus. To determine the influence of desiccation upon these organisms, and also upon the *staphylococcus aureus*, the following experiments were made:

Bouillon cultures of the *bacillus typhi abdominalis*, the *bacillus coli communis* and the *staphylococcus aureus* were roughly dried on threads one centimetre long and then desiccated, a portion being placed in a vacuum, another portion in a desiccator over sulphuric acid, and a third in a closet; all were kept in the dark. The result of the desiccation under the three different conditions is as follows:

Bacillus typhi abdominalis:

Lived in a vacuum from December 30 until July 24, or 207 days. In a desiccator over sulphuric acid from January 3 until July 24, or 213 days.

In a closet from December 18 until July 24, or 229 days.

Bacillus coli communis:

Lived in vacuum from November 29 to May 30, or 183 days.

In a desiccator over sulphuric acid from January 3 until July 24, or 213 days.

In a closet from December 30 until May 30, or 152 days.

Staphylococcus aureus:

Lived in vacuum from November 29 until July 24, or 207 days.

In a desiccator over sulphuric acid from October 25 until April 19, or 178 days.

In a closet from February 13 until July 24, or 162 days.

It will be seen from these experiments that the organisms experimented on endure desiccation for five months, or more, without losing their vitality, and hence the slight evaporation which may have occurred in the insolation experiments, had probably no influence on the results.

It is evident that sunshine must exercise considerable influence in destroying bacteria on the surface of soil, streets, etc., exposed to its influence, but its action is almost confined to the surface, as appears from the results obtained by Esmarch in attempts to disinfect bedding and clothing by this agency. While the light from an incandescent electric lamp has little germicidal effect, that from a powerful arc lamp produces effects similar to those of sunlight, and it has been proposed to use this means to disinfect the walls of infected rooms. The bacillus of tuberculosis appears to be more quickly destroyed by light than the typhoid or the colon bacillus, being killed by exposure to simple diffused daylight in about a week,* and this fact should be borne in mind in advising measures to prevent the diffusion of this organism.

The investigations upon the typhoid and the colon bacillus referred to in this paper, were undertaken as part of a general scheme of inquiry to ascertain the agencies which tend to destroy the typhoid bacillus when it is introduced into a source of water supply, as, for example, into a running stream. An important part of this investigation relates to the influence of the common water bacteria, or of their products, upon the vitality of the typhoid bacillus.

This research was conducted as follows:

* Ueber bakteriologische Forschung: Vortrag in der ersten allgem. Sitzung des X internationalen Congress, 1890.

1. Forty-five varieties of bacteria found in the water of the Schuylkill river were used in the first experiment. Cultures of each organism were made on agar-agar and after attaining a luxuriant growth were sterilized, the reaction was taken, and the medium was again slanted. A set of these tubes was inoculated with the *bacillus typhi abdominalis* and a second set with *bacillus coli communis*.

The object of this research was to ascertain whether the two organisms would grow on media containing the products of the activity of water bacteria. The reaction was alkaline in every tube. The *bacillus typhi abdominalis* and the *bacillus coli communis* lived in every instance, some showing fairly luxuriant growths, while others were only transparent films.

2. In the second experiment, thirty-nine varieties of the water bacteria used in the first experiment were inoculated into tubes each containing 10 cc. of sterilized tap-water and 5 drops of bouillon. Two sets of tubes were made as before, one being inoculated with the *bacillus typhi abdominalis* and the other with the *bacillus coli communis*. To ascertain whether the two organisms under consideration would multiply in the presence of water bacteria, gelatine plates were made for twelve or more days. Both bacilli gave characteristic colonies with each of the water organisms, except two which had apparently an antagonistic effect upon their development. They were both members of the *subtilis* group. In other members of this group this peculiarity was absent.

The typhoid bacillus in several instances outlived its associate organism. In one instance a gelatine plate made from a tube of sterilized water inoculated with the typhoid bacillus and a water bacterium 160 days previously gave characteristic colonies of the *bacillus typhi abdominalis*.

3. To meet the objection that might be

raised to the use of heat for the sterilization of the medium in which the water organisms had grown, the opinion having been advanced that some products of growth are either volatile or rendered inert by high temperatures, flasks each containing 70 cc. of bouillon were inoculated with water bacteria and incubated for from 15 to 20 days. The cultures were then filtered through porcelain, the reaction was taken, and the filtrate was run into sterilized tubes which were inoculated with the *bacillus typhi abdominalis* and the *bacillus coli communis* and then incubated. In each of the thirteen filtrates inoculated the bacilli grew and multiplied for at least four days.

JOHN S. BILLINGS.

ADELAIDE WARD PECKHAM.

CURRENT NOTES ON PHYSIOGRAPHY (I.).

INTRODUCTORY NOTE.

It is proposed to contribute to SCIENCE under the above title a series of notes and comments on recent investigations and current literature concerning physiography, or physical geography in its modern form. A brief statement of the field to be covered may be appropriate at the outset.

Following the plan introduced by Carl Ritter, and popularized in this country chiefly by Arnold Guyot, geography may be defined as the study of the earth in its relation to man. Some prefer to extend this relation to all forms of life. Physical geography may then be defined as the rational study of those features of the earth which must be understood in order to appreciate its relation to man. In deference to the opinions of the majority of the conference on geography, held in Chicago in Christmas week, 1892, physiography is taken as the name of this subject in its modern form, with particular reference to the rational study of the lands, where man dwells. Descriptive geography is an empirical study

that hardly deserves a place in modern teaching. Political geography is undifferentiated history. Commercial geography is the elementary phase of economics. The distribution of plants and animals leads the way to botany and zoölogy; the chief value of this subject coming from the emphasis that it gives to those physical features and conditions of the earth that determine the distribution of life; when it is made a basis for the introduction of classification and terminology, it is misused, for these matters need deliberate study with a method and discipline of their own. The subjects of oceanography and meteorology involve considerations and disciplines so different in many respects from those which characterize the study of the lands that they fully deserve separate names and treatment; but their teachings must be frequently drawn on for use in physiography.

Contributions from many subjects, astronomy, physiology, botany, zoölogy, history and economics, are merged into a single elementary study—geography—in the earlier school years; all are expanded and separately treated in later school years; all deserve to be treated over again afterwards in the broader way characteristic of college teaching; and all include broad fields for investigation in the university.

Physiography being particularly directed to the study of the lands, must of necessity in its higher researches give due consideration to the more minute features of land forms and their development—subjects which recent writers name geomorphology and geomorphogeny—for the sufficient reason that a close understanding of the development of land forms greatly aids the observation, description and recognition of the forms themselves; and that the knowledge thus only to be gained of the forms of the land is essential as a preparation for the careful study of their relations to man and other inhabitants of the earth.

As thus explained, physiography is an outgrowth of geology; and geology, especially field geology, is a necessary preliminary discipline both for those who would undertake the higher study of physiography and for those who would reduce it to the simplest form of expression for early school use.

MEANING OF THE TERM, BASELEVEL.

SINCE the introduction of the term baselevel by Powell twenty years ago, its use has become popular but unhappily its meanings have not been well defined. A subdivision of the work that the word has been made to do now seems desirable. It should be restricted rather closely to its original meanings, and newer terms should be employed for its secondary meanings. Powell originally wrote: "We may consider the level of the sea to be a grand base level, below which the dry lands cannot be eroded, but we may also have, for local and temporary purposes other base levels of erosion, which are the beds of the principal streams which carry away the products of erosion." (*Colorado River of the West*, 1875, 203.) By using a few qualifying adjectives, there need be no confusion between general, local and temporary baselevels. When unqualified, the general baselevel, or sea level, should be understood.

When a region has been baselevelled (the verb being here made from the noun; after the ordinary English fashion), the surface thus produced is often spoken of as a 'baselevel.' For example, J. S. Diller writes: "It is evident that a general baselevel of erosion must have originated approximately at sea level. This is the only position in which a very extensive baselevel can originate. If we now find such a baselevel at a considerable elevation above the sea, its position furnishes evidence that since the baselevel was formed the country has been uplifted." (*Chicago Journal of*

Geology, II., 1894, 33.) Further on in the same article, he writes of the 'deformation of the baselevel.' Although the writer has repeatedly made a similar use of the term, it now seems doubtful if it should be used so freely; and some such word as *peneplain* might serve to replace this extension of the original meaning of baselevel. This is the more advisable, when it is considered how very seldom a region is reduced sensibly to baselevel; how generally a long eroded surface still retains some faint inequality of form which should be expressed in its name.

GEOMORPHOLOGY OF THE SOUTHERN APPALACHIANS.

THE interpretation of the development of geographical features in accordance with the general theory of baselevelling has received two notable contributions during the past year. The first is by Hayes and Campbell on the Geomorphology of the Southern Appalachians (*Nat. Geogr. Magazine*, VI., 1894, 63). The authors recognize the widespread occurrence of more or less fully denuded peneplains at two levels, one of late Cretaceous, the other of late Tertiary date, thus extending the conclusions reached by others farther to the north. They then proceed to measure the amount of deformation that the peneplains have suffered by drawing contour lines upon them. It appears very clearly that the axes of elevation along which these old lowlands have been arched up, coincide closely with the Appalachian axis; thus adding two more dates to the many others at which this line has been the scene of deformation. The tilting of the surface of the deformed peneplains is regarded as of importance in determining the capture and diversion of certain streams by their rivals; this principle being further illustrated by Campbell in a separate article on 'Tertiary changes in the drainage of southwestern Virginia' (*Amer. Journ. Science*, XLVIII., 1894, 21).

GRADED RIVERS.

A RIVER that ceased the active deepening of its valley is by various writers described as having reached its baselevel. Thus A. Winslow writes: "The streams of the prairie country . . . have, in large part, reached base level, and are developing meander plains." (*Missouri Geol. Survey*, VI., 1894; Lead and Zink deposits, 310.) H. Gannett figures a bit of the Great Plains of Colorado as 'near base level,' although the contour lines indicate altitudes of over 4000 feet. (*Monogr. XXII.*, U. S. Geol. Survey, 1893, pl. viii.) Now it is true that streams which have ceased the active deepening of their valleys serve as *local* baselevels for their tributaries—as Powell's original definition stated; but it seems undesirable to speak of these streams as themselves having reached baselevel; still less is the country which slopes down to them necessarily near 'baselevel.' If the term is used in so general a sense as this, then an important feature in the development of rivers will remain undistinguished by any special name, and the attention of readers will not be forcibly brought to it. It is well known that when a river has cut down its valley and reduced its velocity to such a value that its capacity to do work in transporting waste is just equal to the work that it has to do, any further change in the profile of the stream-channel can take place only as fast as a change in the amount of land-waste offered to the streams shall allow. If the amount of waste slowly decreases, as is commonly the case, the stream will slowly assume a flatter and flatter slope (except so far as the development of meanders may lengthen its course and thus retard the deepening of its valley). If an increase in the amount of waste takes place after equality of capacity and task is reached, as sometimes happens, then the stream must aggrade its valley for a time. If the climate of the region changes, a new slope may be

called for. Of two regions, similar in all respects except that one is made of resistant rocks, and the other of weak rocks, the first will develop a stronger relief during its mature dissection than the second. The Great Plains of the West are often referred to as a region of considerable elevation, in which, however, the rivers are unable to cut deep valleys on account of the rapid disintegration of the tributary slopes, and the consequent necessity of maintaining steep-sloping channels in order that the streams may do their work of bearing the plentiful waste of the land to the sea.

All this series of considerations is confused if it is said that a river which has established an equality between its capacity and its task is 'at baselevel.' From whatever profile of slope it began to work on, it has developed a profile of equilibrium, as certain French writers would phrase it; or, following a suggestion by G. K. Gilbert (*Chicago Journal of Geology*, II., 1894, 77), it has graded its slope; it is a graded river; it is almost balanced between degrading and aggrading its valley, and most of its activity may be given to lateral sapping. No better English term than 'grade' has been suggested for the expression of this important idea.

GEOMORPHOGENY OF NORTHERN CALIFORNIA.

THE second contribution to the general subject alluded to above is by A. C. Lawson, in account of the Geomorphogeny of the coast of northern California (*Bull. Dept. Geol., Univ. of Cal.*, I., 1894, 241-242), which students of this new-named subject will do well to consult. Although only the report of a rapid reconnoissance, the paper announces the determination of a well-marked, uplifted and dissected peneplain, in which a fully developed system of subsequent drainage is exhibited on an extensive scale. The district is recommended to students as an inviting field for further in-

vestigation. The author brings out the point that a constructional mass of resistant rocks will never at any stage of its denudation yield a topography that may be reached at certain stages in the denudation of a mass of weaker rocks; and he therefore suggests that in the accounts of topographic development, or geomorphogeny, a factor should be introduced indicative of the rate as well as of the stage of degradation of the region concerned.

THE ESSENTIAL PRINCIPLES OF BASE-LEVELLING.

THE results gained in the two papers mentioned above, and in many other similar articles, are based on the essential principles of baselevelling: Any region must in time be reduced to a nearly featureless peneplain close to sea level; during the progress of its denudation, the forms assumed follow a tolerably well defined sequence, depending chiefly on the structure of the wasting mass; the features and arrangement of the drainage lines are essentially systematic and not arbitrary in their development. A generally accepted corollary of these principles is that a surface of denudation, having faint relief and no control by structure, can be produced only close to its controlling baselevel; and that such a surface represents the peneplain stage, attained close to the end of the cycle of denudation in which it was developed. It is evident that if a plain of denudation can be produced at a considerable altitude above baselevel, and independent of structure, then the conclusions of various investigators regarding land movements, based on the occurrence of elevated, warped or faulted peneplains, must be critically revised. It therefore behooves those who accept and employ the doctrine of baselevelling to examine carefully any alternative hypothesis by which peneplains are explained independently of baselevels.

THE GEOGRAPHICAL EDUCATION OF OUR
TOPOGRAPHERS.

SOME engineers hold the opinion that it is not necessary for a topographer to have an understanding of the forms that he maps; it is sufficient for him simply to record what he sees without knowing its meaning. If all topographers could sketch with minute accuracy, if they all worked on a large scale and without limitation of time, they might perhaps manage to get along without an appreciative knowledge of the subject of their sketching. But the topographers by whom our maps are made cannot as a rule sketch with minute accuracy; and even if they could, their talent would be of little avail, for time could not be given to its use; moreover, maps of a scale large enough for minute accuracy are too expensive to undertake in so vast a country as ours. In many parts of the country the land is hardly worth as much per mile as it would cost to map it in an elaborate manner. Our maps must be made on a relatively moderate scale—seldom more than an inch to a mile; expensive detail cannot be permitted; and very slow work must give way to methods that will give results more rapidly. A great deal of our topographical work must be done by rapid sketching between measured points; the sketching must always be generalized; and every thing that will promote the production of good results from rapid and generalized sketching must be taught to the topographer.

Looking at the subject in this practical manner, there can be no question that an appreciative understanding of topographical features is of great value. Rapid work by a topographer who does not understand the country before him will produce an unappreciative portrait. Generalizations by a surveyor who does not understand the relations of the forms that he generalizes will produce an unsuggestive and inaccurate map. A good understanding of physio-

graphy should therefore be regarded as an essential qualification of a topographer; and schools of engineering should see to it that adequate teaching of this subject is provided for their students.

WINSLOW'S EXPLANATION OF THE MISSOURI
PLAINS.

SUCH an alternative hypothesis is offered by A. Winslow in his recent report on the lead and zinc deposits of Missouri (Geol. Survey of Missouri, Vol. VI., 1894). He describes certain parts of southern Missouri as exhibiting broad expanses of nearly flat land. A 'prominent feature' of the district is 'the steepness of the hills adjacent to the stream valleys' (p. 306). Another part of the same region is a dissected plateau of carboniferous strata, terminating eastward in an irregular escarpment. The even inter-stream uplands of both plain and plateau are not regarded as of constructional origin, for the region has long been above sealevel; the possibility of either upland having once been a smooth peneplain of baselevel erosion is considered and rejected; and the following hypothesis is offered in its stead: "These prairie and plateau plains are primarily due to the fact that the slope of the surfaces has always been and continues slight. . . . Consequently, the flow of the streams has been so sluggish that general atmospheric degradation has nearly kept pace with the corrasion of the streams and formation of the valleys. As a result, the whole surface has been denuded simultaneously. This condition is attributable, first, of course, to the gentleness of the original constructional slope; the horizontality of the stratification has helped to perpetuate it. Secondly as a factor in the production of these surfaces, it is probable that, where streams have corraded so slowly, broad flood plains have been developed at different levels at different times. Thus many flat stretches, which may be removed

from the formative streams, are, perhaps, to be considered as of the nature of terraces marking the flood plains of a past stage of erosion" (p. 322, 323). Change of altitude of the region, or in other words, change of baselevel, is not referred to as essentially involved in the problem.

The plateau surface, sloping to the west and terminating eastward in an escarpment Carboniferous strata, seems to depend on the greater resistance of these strata. It might be called a structural plain; a stripped surface on which general denudation has hesitated by reason of the endurance of the exposed strata, although the streams have deeply trenched it.

With the prairie plains the case is different, for much of their area "is underlain by coal measure rocks, which are readily acted on by sub-aerial agents of erosion" (p. 323). If the streams of the region were not enclosed by steep-sided valleys, but wandered across the plains in channels hardly beneath the general surface level, then it might be admitted that the whole surface would waste away about as fast as the streams degraded their courses. But as the streams are in well-enclosed valleys, it does not seem logical to admit that the inter-stream plains can have wasted as fast as the valley forces. If the streams of the region even now distinctly incise its surface, all the more strongly must they have done so before long continued denudation had reduced its original altitude to its present altitude. The steep valley sides should long ago have been ravined, and the inter-stream plains should thus have been unevenly dissected. If this process had been long in progress, the region might already have reached or passed through the stage of most varied relief—topographical maturity; but it could not have attained an even surface distinctly above the level of its streams. Similarly, it does not seem admissible to suppose that streams, which are now run-

ning in rather narrow, steep-walled valleys, should ever, when still higher above baselevel, have had broad flood-plained valleys, beneath which they have incised the narrow existing valleys, yet without being prompted to this change of behavior by any change of altitude in the region.

A decision as to the origin of these plains must be left to workers on the ground; but opinion as to the sufficiency of the process suggested for their production may be formed by any one who has familiarized himself with the general principles of denudation here involved. In the writer's mind Winslow's hypothesis does not invalidate the generally current principles of the base-leveling theory.

GANNETT'S MANUAL OF TOPOGRAPHIC METHODS.

THE general principle that the topographer should be well trained in physiography is strongly affirmed in Gannett's *Manual of Topographic Methods* (Monogr. XXII., U. S. Geol. Survey, 1893, issued in 1894). The volume contains a concise account of the surveys thus far undertaken in the United States; an account of the map now in progress by the U. S. Geological Survey, this containing much of interest to the geographical reader; and a treatment of the more technical matters of astronomical determination of position, horizontal location, secondary triangulation, sketching, and office work. In the chapter on sketching, there is an interesting discussion of the origin of topographic forms, with illustrations taken from various map sheets in the Survey office; this discussion being introduced 'as an aid in the interpretation of the various topographic forms which present themselves' to the topographer. Here we read the sound statement that "it is in the matter of generalization that the judgment of the topographer is most severely tested. He must be able

to take a broad as well as a detailed view of the country; he must understand the meaning of its broad features, and then must be able to interpret details in the light of those features. Thus, and thus only, will he be competent to make just generalizations" (p. 107).

THE UPLIFT OF THE EXISTING APPALACHIANS.

THE origin of topographic forms has as yet received so small a share of attention from the greater number of field geologists and geographers, and the presentation of the problems involved has as yet gained so little attention from teachers in schools of higher grade that contributions to the subject from a man of Mr. Gannett's experience and qualifications are of great value. Yet in certain parts it seems to the writer that his plan of presentation is open to criticism. He states first that topographic features originate by uplift, by deposition and by erosion. Under the heading of uplift, he writes: "The ridges and valleys of the Appalachian region are the result of uplifts, with numerous sharp folds and faults, which raised at various angles an alternation of hard and soft beds, from which erosion has since carved the existing alternations of ridge and valley" (p. 109). In spite of the qualifications of a preceding paragraph, to the effect that forms produced by uplift are during and since their rise greatly carved by erosion, the reader can hardly acquire a correct understanding of the facts concerning the Appalachian ridges and valleys from Gannett's statement; nor can he easily acquire from the Appalachians an idea of the nature of forms produced by uplift with folding and faulting. Such forms can be illustrated best by the selection of young topographic districts, on which erosion has as yet made little advance. Our western country possesses many and excellent examples of this class. Furthermore, it is no

more allowable to describe the Appalachian ridges and valleys as the 'result of uplifts, with numerous sharp folds and fault' than it would be to associate the fiords of Labrador with the ancient deformation of the old rocks of that region. The Appalachian uplifts with folds and faults have long ago been consumed; the uplift from which the existing ridges and valleys are carved was a broad arching of the region, without folding or faulting of perceptible measure. It is true that the up-arched mass possessed a structure given ages before by folding and faulting; but that more disorderly kind of uplift had little in common with the broad and even uplift of the region by which its present relief was initiated. The essay by Hayes and Campbell, already referred to, gives sufficient demonstration of this important conclusion.

A FRENCH OPINION.

THE following abstract from an essay entitled '*L'age des formes topographiques*' by A. de Lapparent, the eminent geologist (*Revue des questions scientifiques*, Oct., 1894), expresses an opinion concerning the personnel of a topographic corps that is somewhat surprising as coming from France, where we had supposed that the propriety of the military control of official geographical work was unquestioned. De Lapparent writes in effect: The distraction of our professional geographers by the study of arbitrary political boundaries in the early part of this century would have been lessened if the work of detailed mapping had been left to men ready to interest themselves in the many questions provoked by the manifold forms of land relief. Unfortunately the reverse was done in decreeing that cartography should be exclusively a function of the department of war. Up to 1830 there was in France an excellent institution, that of the geographical engineers. Well prepared in the *École polytechnique*, the

officers of this corps devoted themselves entirely to geodesy and topography. Thus occupied they came to have a lively appreciation of the relation between internal structure and external form. Truly, geology was at that time but little advanced, but this productive combination of two orders of studies must have been of mutual advantage, had not an always regrettable decision caused the suppression of the corps of geographical engineers, and the transfer of their duties to the officers of the army staff. Certainly there was no lack of capacity among the latter, but it was nevertheless a capital mistake to entrust a service essentially civil, and even scientific, to military officers who could not devote themselves exclusively to it. Consequently, even though the maps have been well made, there has been a slow advance of what may be called appreciation of topographical form (*l'intelligence du terrain*). Certain of the more sagacious geologists in vain showed how the meaning of topographic form is illuminated when it is studied in relation to internal structure; the divorce of 1830 continued to exercise its unlucky influence, and all the more because other nations, following the example of France, have for the most part identified topographical work with that of the national defense. But a reaction has gradually set in, and to this none have contributed more effectively than the Americans; and here the author goes on to pay a high tribute to the scientific results of our western surveys.

Accepting the correctness of the principles stated by de Lapparent, it follows that our topographers can succeed in their great work only when imbued with a truly scientific spirit. There is small likelihood of this spirit being generally attained so long as engineering schools give so little attention as at present to the study of the great subject on which their topographic art is to be exercised. For this reason, such works

as Gannett's Manual are particularly welcome.

W. M. DAVIS.

HARVARD UNIVERSITY.

THE NEEDS OF METEOROLOGY.

To state a problem clearly is to contribute much towards its solution; to realize one's wants and make them known may bring the needed help; therefore I accept with pleasure an invitation to speak of the needs of meteorology.

Considered as a source of climatological statistics bearing on every branch of human activity, on land and sea, meteorology has been handsomely supported for a century by all governments and scientific organizations. This feature of our work is now carried on by the U. S. Weather Bureau and the State Weather Services with increasing thoroughness from year to year.

Considered as a system for the prediction of storms and weather for a day or two in advance, meteorology has received enthusiastic support by our own and all other nations. We are now doing about all that can be done by the mere utilization of the telegraph and weather map and the cautious application of general average rules, but we are still powerless in the presence of any unusual movement of the atmosphere. Indeed, I do not see that even our West Indian hurricanes are predicted any better to-day than they were in my 'Probabilities' of August, 1871.

Meteorologists can never be satisfied until they have a deeper insight into the mechanics of the atmosphere. Something more is needed than the most perfect organization for observing, reporting and publishing the latest news from the atmosphere. It is not enough to know what the conditions have been and are, but we must know what they will be, and *why so*. We must have a deductive treatise on the laws governing the atmosphere as

complete and rigorous as the 'Celestial Mechanics' of La Place, and this will necessarily be a treatise on the application to the atmosphere of the general laws of force, or what is technically known as the dynamics and thermo-dynamics of gases and vapors. Such a work cannot be written now, nor when written can it be studied successfully unless accompanied by an introductory 'Laboratory manual of physics and hydro-dynamics.'

But the preparation of this latter work demands appropriate laboratory arrangements. I will, therefore, invert the order and say that further progress in meteorology demands a laboratory and the consecration of the physicist and the mathematician to this science. Something like this was started in 1881, by General Hazen, in establishing a 'Study Room,' but it was ruled out by the report of a committee of Congress, and since that day meteorology has more than ever looked to the universities for its higher development. The applications of climatology to geology, physiography, hygiene, irrigation and other matters have been developed, but meteorology itself, the most important and the most complex of all the physical sciences, still remains to be provided for.

The crying need of this science is a *home*, a domicile, a meteorological laboratory, and full recognition as a course in university study.

Without experimentation there is no true progress in the physical sciences.

CLEVELAND ABBE.

WASHINGTON.

CORRESPONDENCE.

A CARD CATALOGUE OF SCIENTIFIC LITERATURE.

EDITOR OF SCIENCE, *Dear Sir*: The efforts which students of the Natural Sciences are constantly making to provide themselves with more complete summaries of the

literature of their various departments all testify to the existence of a wide-spread feeling of dissatisfaction with the existing methods of cataloguing scientific papers and reporting upon the results of scientific research. That this dissatisfaction is felt by none more keenly than by those engaged in the work is shown by the appeal made last spring by the Royal Society to various universities and learned societies for advice as to the feasibility of maintaining by international coöperation a complete catalogue of current scientific literature.

The following circular of the Society, together with the reply of Harvard University to the same, will doubtless be of interest to your readers, and by opening the columns of your journal to a discussion of the subject you will not fail to elicit valuable suggestions with regard to the details of the plan.

In adopting the recommendations of the committee as printed below, the University Council voted "that the Secretary of the Council be instructed to transmit to the Royal Society a letter stating the opinion of this Council, that the expression 'scientific literature' as used in the above recommendation ought to receive a very broad interpretation."

Yours very truly,

H. P. BOWDITCH.

LETTER FROM THE SECRETARIES OF THE ROYAL SOCIETY.

THE ROYAL SOCIETY,

Burlington House, March 22, 1894.

SIR: The Royal Society of London, as you are probably aware, has published nine quarto volumes of 'The Catalogue of Scientific Papers,' the first volume of the decade 1874-83 having been issued last year.

This Catalogue is limited to periodical scientific literature, *i. e.*, to papers published in the Transactions, etc., of Societies, and in Journals; it takes no account whatever of

monographs and independent books, however important. The titles; moreover, are arranged solely according to authors' names; and though the Society has long had under consideration the preparation of, and it is hoped may eventually issue, as a key to the volumes already published, a list in which the titles are arranged according to subject-matter, the Catalogue is still being prepared according to authors' names. Further, though the Society has endeavored to include the titles of all the scientific papers published in periodicals of acknowledged standing, the Catalogue is, even as regards periodical literature, confessedly incomplete, owing to the omission of the titles of papers published in periodicals of little importance, or not easy of access.

Owing to the great development of scientific literature, the task of the Society in continuing the Catalogue, even in its present form, is rapidly increasing in difficulty. At the same time it is clear that the progress of science would be greatly helped by, indeed, almost demands, the compilation of a Catalogue which should aim at completeness, and should contain the titles of scientific publications, whether appearing in periodicals or independently. In such a Catalogue the titles should be arranged not only according to authors' names, but also according to subject-matter, the text of each paper and not the title only being consulted for the latter purpose. And the value of the Catalogue would be greatly enhanced by a rapid periodical issue, and by publication in such a form that the portion which pertains to any particular branch of science might be obtained separately.

It is needless to say that the preparation and publication of such a complete Catalogue is far beyond the power and means of any single society.

Led by the above considerations, the President and Council of the Royal Society have appointed a committee to enquire into and

report upon the feasibility of such a Catalogue being compiled through international coöperation.

The committee are not as yet in a position to formulate any distinct plan by which such international coöperation might be brought about; but it may be useful even at the outset to make the following preliminary suggestions:—

The Catalogue should commence with papers published on or after January 1, 1900.

A central office or bureau should be established in some place to be hereafter chosen, and should be maintained by international contributions, either directly, that is by annual or other subsidies, or indirectly, that is by the guarantee to purchase a certain number of copies of the Catalogue.

This office should be regularly supplied with all the information necessary for the construction of the Catalogue. This might be done either by all periodicals, monographs, etc., being sent direct to the office to be catalogued there, or by various institutions undertaking to send in portions of the Catalogue already prepared, or by both methods combined.

At such an office arrangements might be made by which, in addition to preparing the Catalogue, scientific data might be tabulated as they came to hand in the papers supplied.

The first step, however, is to ascertain whether any scheme of international coöperation is feasible and desirable. The committee accordingly is desirous of learning the views upon this subject of scientific bodies and of scientific men.

We, therefore, venture to express the hope that you will be so good as, at some early opportunity, to bring the matter before the Harvard University and to make known to us, for the use of the committee, the conclusions arrived at concerning it.

Should the decision you report be in any way favorable to the scheme, may we fur-

ther ask you to communicate to us, for the use of the committee, any suggestions which you may think it desirable to make; as to the best methods of inaugurating a scheme; as to the constitution and means of maintenance of the Central Office; as to the exact character of the work to be carried on there; as to the language or languages in which the Catalogue should be published, and the like?

We are, your obedient servants,
(Signed) M. FOSTER, *Secretary R. S.*
RAYLEIGH, *Secretary R. S.*
J. LISTER, *Foreign Sec. R. S.*

REPORT OF THE COMMITTEE OF THE UNIVERSITY COUNCIL APPOINTED TO CONSIDER THE COMMUNICATION OF THE ROYAL SOCIETY RELATING TO A CATALOGUE OF SCIENTIFIC PAPERS TO BE MADE BY INTERNATIONAL COÖPERATION.

To the University Council of Harvard University:—

The committee of the University Council, to whom was referred the accompanying circular of the Royal Society, respectfully submits the following report:

The committee finds itself fully in sympathy with the desire of the Royal Society to improve the methods of cataloguing scientific literature, and is distinctly of the opinion that the establishment of such a catalogue, to be compiled through international coöperation, is both desirable and practicable.

To determine in what way this result can be best attained, it will be well to consider what are the defects of existing methods, and what are the requirements which an improved system may be reasonably expected to fill.

Bibliographical catalogues and indexes are generally defective in one or two ways. Either they present simply a list of titles which often convey an inadequate, and sometimes a misleading idea of the contents

of the articles catalogued, or they appear, like the various annual reports, so long after the publication of the articles which are reported upon that they lose a great part of their value as guides to current literature. A third defect is common to all existing catalogues, viz., that of necessitating a reference to a number of separate volumes whenever the literature of several years is to be sought for.

It is evident that some form of *card catalogue* can alone remedy these defects, so that the practical question is: How can a card catalogue of current scientific literature be best established and maintained? The requirements of such a catalogue may be stated as follows:—

1. It should appear promptly—if possible, simultaneously with the book or article catalogued.
2. It should furnish an accurate description of the purport of the book or article.
3. It should be readily accessible to all persons interested in the literature catalogued.

It seems probable that these requirements may best be met by the coöperation of a central bureau with the various publishers and editors of scientific literature in issuing with each book and with each number of every periodical a set of cards of standard size and type, each card to exhibit for a book, or for a single article in a periodical:—

1. The name of the author.
2. The title of the book or article.
3. The date, place, and house of publication of the book, or the title, volume, and page of the periodical in which the article appears.
4. A brief statement, not to exceed eight or ten lines, to be prepared by the author himself, setting forth the general purport of the book or article, so as to furnish the necessary data for cross references.

Each card should be in duplicate to permit of arrangement according to subject or

author, or both if desired, and additional cards should be issued whenever the character of the title necessitates cross references. A card when printed would present somewhat the following appearance:*

Calderwood, Henry. Evolution and Man's Place in Nature. Macmillan & Co., London and New York. 1893. pp. 349. sm. 8°.

Summary :

.....

.....

.....

.....

.....

.....

.....

Gourlay, F. The Proteids of the Thyroid and the Spleen. Journal of Physiology. 1894. Vol. xvi. p. 23-33. Plate II.

Summary :

.....

.....

.....

.....

.....

.....

.....

The dimensions and texture of the card should be determined by careful comparison of the cards already in use in the principal libraries of the world.

Space should be left at the top of the card for writing such words as may be desired for cross references. This could best be done by each person for himself, as there would necessarily be much difference of opinion as to the number and character of the cross references desired. Furthermore, subscribers of different nationalities would wish to catalogue the same subject under different headings, *e. g.*, an article on the spleen would be catalogued by a Frenchman under *rate* and by a German under *Milz*.

* The size is here reduced.

If thought desirable, the type used in printing the cards could be kept set up till the end of the year, and then, by arranging the material according to subjects, an annual report in book form could readily be published.

A central bureau charged with the work above outlined could very properly be established under the auspices of the Royal Society. In this central office subscriptions could be received from libraries and individuals for the cards relating to the articles published in certain journals, or to the literature of certain departments of science, and the subscriber would thus receive, in weekly instalments, a complete card catalogue of all the literature in his own line of work. The cards thus received could be arranged by each subscriber so as to form the sort of card catalogue best adapted to his own needs.

Although in this scheme the greater part of the work, including the printing of the cards, would be done in a central office, yet the coöperation of the publishers could not well be dispensed with, for from them must be obtained the summaries prepared by the authors, which form an essential feature of the scheme. No difficulty need be anticipated in obtaining such summaries; for it would be the interest of the writers to furnish them, and no one could prepare them so easily and correctly as the writers themselves.

A central office with this function would readily secure the coöperation of libraries and learned societies throughout the world; and to an undertaking thus endorsed the publishers of scientific literature would doubtless lend their aid, since they would find in it a means of advertising their business. The support of such an office could be provided for at the outset by international subscription; but it would doubtless in a short time become self-supporting, since portions of the total catalogue would be needed

not only in every public library, but on the study table of every serious student in every department of science.

The above report is submitted not as an elaborated plan, but as a suggestion of the end to which effort should be directed. Your committee would further express the hope that some plan may be put into operation at an earlier date than the year 1900, the time suggested in the circular of the Royal Society.

In accordance with the views above set forth the committee respectfully recommends the adoption by the University Council of the following votes:—

1. That, in the opinion of the University Council, the establishment of a catalogue of scientific literature to be maintained through international coöperation is both desirable and practicable.

2. That a copy of this report be transmitted to the Royal Society as the suggestion of a way in which this plan may be successfully carried out.

3. That the Corporation be requested to contribute a suitable sum toward the carrying-out of this enterprise, provided the plan finally adopted by the Royal Society shall appear to the University Council to be practicable.

HENRY P. BOWDITCH, *Professor of Physiology,*
Chairman.

FREDERICK W. PUTNAM, *Peabody Professor of American Archaeology and Ethnology.*

NATHANIEL S. SHALER, *Professor of Geology.*

EDWARD C. PICKERING, *Paine Professor of Practical Astronomy.*

JOHN TROWBRIDGE, *Rumford Professor and Lecturer on the Application of Science to the Useful Arts.*

WILLIAM G. FARLOW, *Professor of Cryptogamic Botany.*

HENRY B. HILL, *Professor of Chemistry.*

EDWARD L. MARK, *Hersey Professor of Anatomy.*

WILLIAM T. COUNCILMAN, *Shattuck Professor of Pathological Anatomy.*

IRA N. HOLLIS, *Professor of Engineering.*

HUGO MÜNSTERBERG, *Professor of Experimental Psychology.*

WILLIAM F. OSGOOD, *Assistant Professor of Mathematics.*

JUNE, 1894.

SCIENTIFIC LITERATURE.

Systematic Survey of the Organic Colouring Matters. By Drs. G. SCHULTZ and P. JULIUS.

(Translated and edited, with extensive additions, by ARTHUR G. GREEN, F. I. C., F. C. S., Examiner in Coal-tar products to the City and Guilds of London Institute.) London and New York, Macmillan & Co. 1894. 4°, pp. viii + 205. Price, \$5.00.

The industry of the organic coloring matters has within a comparatively few years grown to enormous dimensions, and it is becoming difficult even for the specialist in organic chemistry to keep track of the new products. In this valuable book a carefully classified list is presented of 454 dye stuffs which have been patented, and many of these are now in extensive use. All of them are derived indirectly from coal-tar. Under each dye we find the common name, together with other names sometimes used; the scientific name; the empirical formula; the constitutional formula; the method of preparation; the year of discovery; the name of the discoverer; reference to the patents granted; behavior with reagents; shade and dyeing properties, and method of employment. The original German edition is so well known, and it has acquired such a high reputation that any words of praise for the book would be superfluous. The translator's work seems to have been done with care, and he has not only furnished a translation of the original, but brought the work up to date, that is to say, up to the date of publication, for it must be borne in mind that a book treating of organic coloring matters bears to the general subject somewhat the relation that an instantaneous photograph bears to the rapidly moving object which it attempts to represent.

The authors tell us that: "The average quantity of gas tar worked up per annum is given at 350,000 tons for England, and 530,000 tons for the whole world, whilst the

quantity of coke-oven tar, though constantly increasing, probably does not at present exceed 50,000 tons. It may be expected, however, that with the more general introduction of electricity for lighting purposes and the consequent diminution of the supply of gas tar, the coke-oven tar will eventually become the main source of our aromatic hydrocarbons." To this it should be added that the increasing use of 'water-gas,' in this country at least, is decreasing the supply of coal-tar, so that the time is certainly approaching when it will pay to collect the tar from the coke-ovens.

The translator expresses the hope "that this work will be found valuable not only to the technical chemist, but also to the dyer, analyst, merchant, patent agent, etc., and in fact to every one concerned with the production, handling, or use of the coal-tar colours." His hope is undoubtedly well founded. He might have added the patent lawyers, many of whom have learned to rattle off their 'ortho,' 'meta,' 'para' with a facility that would put many a modest chemist to the blush. IRA REMSEN.

Elementary Lessons in Electricity and Magnetism. SYLVANUS P. THOMPSON. New York, Macmillan & Co. 1894. Pp. 628. Price, \$1.40.

The first edition of this book appeared in 1881. It at once became immensely popular, and deservedly so, on both sides of the Atlantic. The author combined in a rare degree the three principal requisites for the preparation of a good text-book. He was himself a widely known scholar and investigator in the department of science specially treated; he was more than ordinarily accomplished in the art of exposition, and he was an experienced and successful teacher. His possession of these qualifications in undiminished magnitude is evidenced in the preparation of this new edition now offered to the public, which is the original work in plan, but entirely revised and largely re-

written, with an enlargement of scope sufficient to embrace the important additions to the science which have been made during the past fifteen years. To enable this to be done without undesirable condensation, the size of the volume has been somewhat increased. Indeed, one of the larger merits of the plan of the book is to be found in the conscientious retention of the long known and well established principles and facts of the science, to neglect which for the newer and more novel developments is a temptation to which too many authors of text-books in physical science have yielded. While retaining all essential 'fundamentals,' Professor Thompson has found place for the presentation of all of the essentials of recent discovery, and while this has been done with conciseness it has also been done with that clearness and logical appropriateness for which the writings of this author are justly celebrated. The wonderful results of the study of alternating currents and alternating current machinery are well presented in this edition, as are recent advances in both theory and experiment due to Hertz, Fitzgerald, Boltzmann, Lodge and others. At the end is an excellent series of questions, classified as to the chapters of the books to which they refer, which cannot fail to add much to the value of the book in use, especially for those who study without an instructor. In fact, as an 'all around' elementary text-book in electricity and magnetism it will be difficult to find another in the English language that is superior or even equal to this.

T. C. M.

The Birds of Eastern Pennsylvania and New Jersey, prepared under the direction of the Delaware Valley Ornithological Club. By WILMER STONE. Philadelphia, 1894. 8°, pp. vii+185.

Eastern Pennsylvania has long been a favorite field for lovers of birds. Audubon, Wilson, Nuttall, Cassin, Peale, Woodhouse, Gambel, Bonaparte, Heerman, Haldeman,

Ord, Baird and Trumbull may be numbered among the contributors to its ornithological literature. Aside from general works and special or local papers, three publications have been devoted to the birds of this particular area: (1) Barton's *Fragments of the Natural History of Pennsylvania*; (2) Trumbull's *Birds of East Pennsylvania and New Jersey*; (3) Witmer Stone's *Birds of Eastern Pennsylvania and New Jersey*. Barton's 'Fragments' is a rare folio printed in Philadelphia in 1799, and is something of a curiosity. Trumbull's list is a carefully annotated and attractively illustrated catalogue published in Glasgow, Scotland, in 1869, and reprinted in America. Stone's 'Birds of Eastern Pennsylvania and New Jersey' is a large octavo published by the Delaware Valley Ornithological Club in December, 1894. It is a thoroughly modern work, abounding in exact data and authorities, and based largely on the field observations of Mr. Stone and other members of the Delaware Valley Ornithological Club—evidently a very active organization. It is divided into two principal parts: An essay on the Geographic Distribution and Migration of Birds; and a Systematic Annotated List of the Birds of the region. To these are added a bibliography and an index. The chapter on Geographic Distribution is subdivided into general and local parts. The general part is weak, and in the references cited some of the more recent and important papers are overlooked. The local part is excellent and gives ample evidence of Mr. Stone's familiarity with the somewhat diverse physical and faunal characteristics of the region. Some idea of its scope may be had from the headings: The Maritime Marshes, the Pine Barrens, the Cedar Swamps, the Lowlands of Pennsylvania, the Delaware Valley, the Susquehanna Valley, the Interior Uplands, the Appalachian District, the Alleghany and Pocono Mountains. This part is accompanied by a curious col-

ored map which might be termed a physico-faunal map of Eastern Pennsylvania and New Jersey.

The Canadian or Boreal element in the fauna is restricted in Pennsylvania to "the tops of the highest mountains and the elevated plateau region, where the deep hemlock forests, with their cool brooks and dense shade, still remain undisturbed. The passage from the Alleghanian to the Canadian zone is here, as a rule, remarkably distinct, as the more northern birds keep strictly to the virgin forest." The settlement of the region has proved particularly destructive to the Canadian species. It is melancholy to be told that "where the forest has been removed the Canadian species for the most part disappear, and judging from present indications, it would seem that this element in our fauna, which once undoubtedly extended over a much greater area than at present, may soon almost entirely disappear, as the lumbermen year by year encroach upon the forest tracts."

The chapter on *Bird Migration* is full of interest and replete with new information respecting the region studied.

In the Systematic part no less than 352 species are recorded on good evidence as occurring within the area embraced by the catalogue. A new departure is here introduced which more pretentious works would do well to follow. Instead of the much abused term '*Habitat*' the '*Breeding range*' and '*Winter range*' of each species are given. Mr. Stone is to be congratulated upon the distinction of being first to inaugurate this reform, which is bound to come into general use in the near future. Another improvement that might be made in all lists of birds is the transfer of accidental stragglers from the body of the work to a special list at the end. Since such extra-limital species form no part of the proper fauna of a region, why should they be included among the regular inhabitants? C. HART MERRIAM.

Visitor's Guide to the Local Collection of Birds in the Museum of Natural History, New York City. By FRANK M. CHAPMAN. 1894. 8°, pp. 100. 15 cents.

One of the best and most attractive local bird lists that has ever appeared in America has been recently issued from the American Museum of Natural History, New York. While it bears the misleading title *Visitor's Guide*, only a glance is necessary to see that it is much more. It is in reality a compact treatise on the birds known to occur within 50 miles of the great metropolis.

The author, Mr. Frank M. Chapman, prefaces the list proper by 12 pages of interesting and important matter respecting the physical and faunal aspects of the region, and the birds that are found there at different seasons. The area covered by the list is unusually rich in birds, no less than 348 species being recorded as occurring within its limits. This richness, as stated by Mr. Chapman, is due in part to the circumstance that two faunas—the *Alleghanian* (or eastern division of the Transition Zone) and *Carolinian* (or eastern division of the Upper Austral Zone) meet within its boundaries, and in part to the natural advantages of the region. "Our sea-coast, with its sandy beaches and shallow bays; our rivers, creeks and ponds, with their surrounding grassy marshes; our wooded hillsides and valleys; our rolling uplands and fertile meadows, offer haunts suited to the wants of most birds. Again, our coast-line and the Hudson River Valley form natural highways of migration regularly followed by birds in their journeys to and from their summer homes."

The paper is a model of its kind and should be in the hands of all interested in the birds of New York and vicinity. It is bountifully illustrated by cuts of birds borrowed from Coues' '*Key*,' to which are added several full-page plates of groups in the American Museum.

C. HART MERRIAM.

Outline of Dairy Bacteriology. By H. L. RUSSELL, University of Wisconsin. Published in Madison, Wisconsin, 1894. Pp. vi+186.

There is no better indication of the rather remarkable advance that has been made in recent years in bacteriological matters not connected with diseases than the publication of a text-book upon dairy bacteriology. That there should be demanded for classes in dairy schools a text-book describing the various phenomena connected with bacteria in their relation to dairy matters is rather surprising when we consider the fact that dairy bacteriology itself is the result of experiments of the last very few years. Prof. Russell has attempted in this little book of about 180 pages to give an outline of the present knowledge of the relation of bacteria to milk and all its products. The book is designed originally for his classes in a dairy school, and is, as its title indicates, only an outline, not involving any critical scientific discussions. As an outline, however, it is quite complete and the treatment is satisfactory. The book will be of use not only in dairy schools, but to all who are interested in matters connected with milk or butter supply. It will also be found useful to nurses and physicians who desire a knowledge of some of the recent discussions in connection with milk bacteriology and its relations to diseases.

H. W. C.

The nature and distribution of attraction-spheres and centrosomes in vegetable cells.—JOHN H. SCHAFFNER. Bot. Gaz. Nov. 1894.

The author studied centrosomes found in root tips of *Allium cepa* L., *Vicia faba* L., *Tradescantia rosea* L., also in the resting cells of the epidermis of *Allium cepa* bulb scales and in the walls of *Lilium longiflorum* ovaries. The usual methods for preparing and staining the material were adopted. In addition the author used a stain suggested by

Prof. Newcombe. It is called the iron-tannin-safranin stain and consists of the following solutions: 1, 1% aq. sol. of ferrous sulphate; 2, 5% aq. sol. of tannic acid; 3, alcoholic solutions of anilin-safranin; 4, aq. sol. of picro-nigrosin. The sections are placed for thirty to forty minutes in the iron solution, washed, then placed for the same period in the tannic acid solution; again washed and replaced for a few minutes in the iron sol. After washing again they are placed in the safranin for thirty minutes; then fifteen minutes in the picro-nigrosin. This method is said to give good results.

The special results of the investigations may be summarized as follows: (1.) Centrosomes and attraction spheres are present in non-reproductive as well as in reproductive vegetable cells. (2.) In phanerogams there are two of these bodies for each resting nucleus. (3.) When the nucleus prepares to divide, one or both of the centrosomes migrate to take their position at the poles of the future spindle. (4.) Subsequently they immediately begin to divide. The division is complete in the prophase of the mother nucleus. (5.) After their migration the spheres remain at the poles of the nuclear spindle and do not change their position until the beginning of the following division. (6.) Centrosomes are persistent.

One plate and a list of thirty-three valuable references accompany the article.

ALBERT SCHNEIDER.

NOTES AND NEWS.

THE ELIHU THOMSON PRIZE.

THE Elihu Thomson prize of 5,000 francs has been awarded to Dr. Arthur G. Webster, of Clark University, Worcester, Massachusetts. The history of this prize is, briefly, as follows:—

In 1889 the City of Paris offered a series of prizes for the best 'electric meters,' it being required that certain conditions should

be satisfied, to be determined by an exacting practical test. The first prize, 5,000 francs, was awarded to Professor Elihu Thomson, who submitted the well known Watt-meter devised by him. Wishing to encourage investigation of certain theoretical questions Professor Thomson donated the prize for the establishment of a new competition, the subjects to be considered and the prize to be determined by a committee which consisted of J. Carpentier, Hippolyte Fontaine, Hospitalier, Mascart, A. Potier and Abdank-Abakanowicz. Four subjects for investigation and discussion were selected, and it was announced that competing memoirs must be submitted on or before September 15, 1893. Four memoirs were submitted to the committee; one of these was written in German, one in French and two in English. The two latter, numbered respectively three and four, related to the same subject, namely, the determination of the period of electric oscillations. On examining the memoirs the committee reported that it 'considered memoir number four to be worthy to receive the prize established by Professor Elihu Thomson,' and expressed the hope that the author will be encouraged to continue his beautiful researches.

At the same time they express their regret that they have not available another prize of the same value which they would be glad to award to memoir number three. When their desire in this respect was made known, Professor Thomson and the French and English Thomson-Houston Electric Companies joined in offering another 5,000 francs, which was awarded to the author of memoir number three. On opening the sealed envelopes containing the authors' names, it was found that memoir number four, for which the first prize had been awarded, was prepared by Dr. Webster, and number three was the joint product of Oliver Lodge and Glazebrook.

The title of Dr. Webster's memoir was *An Experimental Determination of the Period of Electric Oscillations.*

He is to be congratulated upon so signal a success, and it is especially gratifying that an American should have come out in the lead in competition with the two distinguished Englishmen who contested with him, and especially so as their work and his were upon the same subject.

ENTOMOLOGY.

DR. MCCOOK is to be warmly congratulated on the successful issue of the third and final volume of his *'American Spiders and their Spinning Work,'* which has appeared four years after the second volume. The author is more at home in his delineation of the outdoor world than in systematic work, with which this volume is mainly concerned, yet he has applied himself to this task with commendable zeal and success and describes 123 species and 30 genera. Apparently (as the table of contents curiously shows) he had intended to carry his work beyond the 'orb weavers,' but his courage or his time gave out as he saw his work grow to portentous dimensions. We have to thank him for thirty large and careful plates of spiders colored, besides a mass of structural details; they will greatly facilitate future study. The price of the complete work is now justly advanced to \$50. Unhappily the title page is marked 1893, though the preface is dated in July, 1894, and the volume was not issued until December, 1894.

MR. AND MRS. PECKHAM have given us (*Trans. Wisc. Acad.*, X) a new series of their admirable experiments with spiders in a paper on their visual powers and color sense; they "prove conclusively that *Attidae* see their prey (which consists of small insects) when it is motionless; up to a distance of five inches; that they see insects in motion at much greater distances; and that they see each other distinctly up to at least

twelve inches"; they are guided by sight rather than by smell. The experimenters are further "of the opinion that all the experiments taken together strongly indicate that spiders have the power of distinguishing colors."

CERTAINLY the University of California Entomological Society has done a unique thing in issuing from Berkeley, Cal., as a Californian journal of entomology *'The Entomologists' Daily Post Card'* at \$2.00 a year. A card of regulation size and color is printed on both sides in clear type, leaving a meagre space for an address. The number before us contains an editorial on note taking, part of a list of species in Edwards's last catalogue of butterflies, and a portion of a tabular key to the genera of *Nymphalidæ*. It is a curious venture.

In a recent paper on the Siphonaptera (*Proc. Bost. Soc. Nat. Hist.*, XXVI., 312-355) Dr. A. S. Packard gives an excellent resumé of published observations on the embryology, postembryonic history and anatomy and the adult structure of the fleas, adding new data from his own preparations and numerous figures. He is led to regard them as forming a distinct order standing nearer the *Diptera* than any other, but with many points of relationship to the *Coleoptera*.

HANSEN gives in English (*Ent. tidskr.* XV., 65-89, pl. 2-3) an important paper on the structure and habits of *Hemimerus*, a *Platypsylla*-like insect infesting rats in Africa, and which had previously been studied only from dried material. Saussure in particular had published a long memoir upon it, founding upon it a new order *Diploglossata* from its possessing, as he thought, a second labium. Hansen shows that this does not exist (it is difficult to understand how the figures of Hansen and Saussure can have been taken from the same

kind of insect) and he concludes that "Hemimerus belongs to the Orthoptera, constituting a separate family very closely allied to the Forficulina." He shows from his dissections that the insect is viviparous, bringing forth one young at a time.

THE COOLING OF HOSPITALS.

DR. MORRILL WYMAN, of Cambridge, Massachusetts, has published in the Proceedings of the American Academy of Arts and Sciences, Vol. 30, page 482, an interesting paper giving the results of some experiments made in the Cambridge Hospital, in which the air admitted to the wards in warm weather was cooled by passing it through pipes in which cold water was circulating, these pipes being the same as those used for warming the air in the winter by the circulation of hot water. In one experiment the external temperature was at 83° F.; there was no wind and the patients were suffering from the heat. The temperature of the water as it entered the cooling pipes was 57 to 58 degrees. An electrical fan 36 inches in diameter, making five hundred revolutions a minute, forced about 10,200 cubic feet of the warm outer air through these pipes into the ward, which contained 21,000 cubic feet. In an hour the air entering the ward was at 71° F., and the comfort of the patients was manifestly improved. But the cooling surfaces were not only the ten cooling coils of 30 square feet each, but also the four walls of the air chamber beneath the ward, being about 3,300 square feet of surface, and it is estimated that the cooling power of the coils was about one-tenth that of the walls. A month later the heat of the external atmosphere was greater and the fan was more constantly in motion; the temperature of the air chamber had increased, and that of the water had risen to 70°. The quantity of water required for the circulation was large and expensive, and it was therefore shut off. But the same

amount of ventilation was continued, the air passing through the air chamber. During the summer the ward temperature gradually rose until it differed but little from that of the open air. Nevertheless, the comfort given to the patients and nurses was immediate and decided, and there was a decided feeling of freshness and freedom of air.

Dr. Wyman points out that we can do little towards lowering the temperature of the air in hot weather in the volumes required for the ventilation of a hospital. It is a question of the rate of evaporation from the perspiring surface, which is governed in a great measure by the velocity of the air coming in contact with that surface, and this is a factor which by art it is possible to control. If we try to cool the air before it enters the ward, it must be remembered that air absolutely humid, when brought into contact with warmer air also saturated, will cool the latter, which will approach dew-point, and if its moisture is condensed into visible vapor will give out heat. "Evaporation consumes heat, condensation liberates heat." "To give comfort during the excessive heats of summer the sick require three or four times the air needed for satisfactory ventilation in winter. It required 400,000 cubic feet an hour for our sixteen patients, and yet while this large quantity was passing through the ward it was only known, except at the registers, by the accompanying sense of freshness and pleasant coolness; it was never felt as a draught."

"The experience of the Cambridge hospital leads to these two conclusions: first, that fresh air directly from the open, in the quantity and manner there supplied, can be made to give great comfort to the sick during the heats of summer; and, secondly, that previous cooling of the air so supplied is difficult and practically useless."

PITHECANTHROPUS ERECTUS.

PROFESSOR MARSH has contributed to the February number of the *American Journal of Science* an account illustrated by plates of the discovery by Dubois described in SCIENCE (January 11) by Professor Brinton. A writer in *Nature* (January 24) under the initials R. L. (Professor Lankaster) holds that the remains are human, the skull being that of a microcephalous idiot. Professor Marsh writes:—

"The brief review here given of the main facts relating to this discovery, together with the figures reproduced from the memoir, will afford the reader some idea of the importance of this latest addition to the known allies of primæval man, if not to his direct ancestry. Whatever light future researches may throw upon the affinities of this new form that left its remains in the volcanic deposits of Java during later Tertiary time, there can be no doubt that the discovery itself is an event equal in interest to that of the Neanderthal skull.

"The man of the Neander valley remained without honor, even in his own country, for more than a quarter of a century, and was still doubted and reviled when his kinsmen, the men of Spy, came to his defense, and a new chapter was added to the early history of the human race. The ape-man of Java comes to light at a more fortunate time, when zeal for exploration is so great that the discovery of additional remains may be expected at no distant day. That still other intermediate forms will eventually be brought to light no one familiar with the subject can doubt. Nearly twenty years ago, the writer of the present review placed on record his belief that such missing links existed, and should be looked for in the caves and later Tertiary of Africa, which he then regarded as the most promising field for exploration in the Old World. The first announcement, however, has come from the East, where large anthropoid apes

also survive, and where their ancestors were doubtless entombed under circumstances favorable to early discovery. The tropical regions of both Asia and Africa still offer most inviting fields to ambitious explorers."

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES.

THE section of Geology and Mineralogy of the New York Academy of Sciences met on Monday evening, January 21, and listened to a paper by Prof. R. S. Woodward, of Columbia College, on the *Condition of the Interior of the Earth*, of which the following is an abstract. The two envelopes of the earth, the atmosphere and the ocean are important factors in the problem of the interior, and yet we know less of the condition of the outer atmosphere than of the inner earth. The atmosphere's shape we can calculate, with some approximation to the truth, as an oblate spheroid, whose polar radius is 5.4 times the earth's radius, and whose equatorial radius is 7.6 times the latter. This shape is determined by centrifugal force and gravity. Its bulk is 310 times that of the earth, but its mass is only one-millionth that of the latter. If we speak of the latter as 6642×10^{18} tons we can get some conception of the mass of the atmosphere, and of its extreme tenuity in the outer portions.

Our inferences regarding the interior of the earth rest chiefly upon four facts, viz.

1. Its shape and size, which are known with great accuracy.
2. Its surface density, 2.6.
3. Its mean density, 5.58, which is probably accurate within two units in the second decimal place.
4. The precession ratio $\frac{C-A}{C}$, in which C is the moment of inertia of the earth with respect to the polar axis, and A is the moment of inertia with respect to an equatorial axis.

These facts limit the distribution of the earth's mass. The density of the mass must

increase from the surface toward the center. Various laws of its increase have been proposed, of which that of Laplace seems to be on the whole the most plausible.

It is important to appreciate that the strata rest upon one another substantially as if fluid, because the arch of the crust is so flat. The compressive stress on any portion considered as a keystone is 30 times the crushing strength of steel, and 500-1000 times that of granite and limestone, whence it follows that the earth is practically in hydrostatic equilibrium. It also follows that the pressures in the interior are excessive, and that at the center the pressure is about 3,000,000 atmospheres. The earth is 'solid,' as the word is used by Lord Kelvin, that is, it has no cavities below a comparatively shallow depth. The explanations of the changes of latitude lately advanced and based on internal hollows in which loose matter rolls around are absurd. There is perfect continuity of matter, and there is only fluidity when for some local cause the pressure is somewhat relieved. As Major Dutton has shown, the transmission of vibrations from the centrum of the Charleston earthquake indicated a medium nearly as homogeneous as steel.

Geologists have had to account for movements of the crust, such as subsidence, elevation, crumpling, folding, etc. Two elementary forces are necessarily appealed to. The first is *Gravity*; the second that due to the *Earth's Internal Heat*. The idea of the earlier geologists that the earth cooled and contracted and hence caused the disturbances has been mostly relied on as an explanation, but for the last ten or fifteen years it has been felt to be insufficient. The idea of Babbage and Herschel that loaded areas, or areas of sedimentation, sink and crumple up the adjacent areas as mountains, tending thus to renew and perpetuate regions of upheaval, has also had believers. This has had its best formulation in the re-

cent doctrine called *isostasy*, which regards the earth as a body in essentially hydrostatic equilibrium, and as balancing inequalities of pressure by subterranean flow. The speaker regarded this doctrine, however, as insufficient in that it furnishes no start and tends to run rapidly down. We need secular contraction to keep isostasy at work. The earth's internal heat is the great store of energy available for this purpose. How to explain the earth's internal heat is a hard and dark problem. The nebular hypothesis, first outlined in Leibnitz's *Proto-gæa* has been most widely believed. The critical stage in this method of development came when convection ceased and the sphere was all at the same temperature, the stage usually called *consistentior status*. Then came the formation of a crust and the beginning of geological phenomena as usually discussed. The speaker had reason to question the reliability of the nebular hypothesis and whether the earth had ever been gaseous, etc. An origin for the globe and an explanation of its heat are perhaps as well to be found in the collision of meteoric bodies.

The time that has elapsed since the *consistentior status* has been an interesting subject for computations, and widely varying estimates have been made. Lord Kelvin in 1862, on very questionable data, placed the limits of geological phenomena at 20,000,000-400,000,000 years in the past. On the same line, Tait estimated 10,000,000, but it was doubtless true that in England the weight of Kelvin's authority had fettered geological thought in the last thirty years to too narrow limits of time, for no geologist of eminence had questioned his results. Yet within a month Lord Kelvin has raised his upper limit to a possible 4,000,000,000. All must appreciate that if the data are unreliable, the finest processes of mathematics will lead to no certain result:

The speaker concluded that to secular

cooling must be attributed the principal motive force. The main criticism raised against it is its insufficiency; but George Darwin has shown that as a cause it can be mathematically shown to be able to produce results at least of the same order as those observed. In the speaker's estimation it is probably sufficient, although the heat radiated is a very difficult thing to measure in a reliable way. Our data are all from the continents, and they have not been obtained in sufficient quantity. The oceanic areas are necessarily unobserved.

In discussion Professor Kemp stated that attention had been naturally been drawn to the interior of the earth in the endeavor to explain, first of all, the contrasts of the continental elevations and the oceanic abysses, and secondly, the crumplings, foldings and faults of mountainous regions. Herschel's explanation, while rational and simple on the face of it, is inapplicable because it is the area of sedimentation, subsidence and 'overloading' that later on is upheaved in the mountains, and this apparent contradiction is the great difficulty. He also referred to the measures of rigidity of the crust, to the remarkable localization of the yielding along narrow lines when it did come, and to its great effects and relatively short duration. He asked Professor Woodward also to touch on the slowing up of the revolution of the earth and the consequent readjustment of the spheroid to the loss of centrifugal force, an idea advanced some years ago by W. B. Taylor.

In reply Professor Woodward admitted that the questions were old and very difficult ones, and that for the mountains he had no explanation to advance. He spoke of the mountainous protuberances as measures of the rigidity, and yet this must be qualified by the statement that according to isostasy and to recent pendulum observations they appear to be somewhat lighter under the surface. As to the slowing up of rotation and

loss of centrifugal force, the idea was an important and valuable one, but it did not appear to be sufficient to account for the results.

Professor Rees referred to the recent observations on changes in latitude made under his direction, and to certain factors that entered into the calculations which would throw light on the question.

Professor Hallock brought up the recent results of experiments on the gyration of liquids as bearing on the question and proving that a fluid set in rapid rotation continues to gyrate long after the enclosing vessel ceases. The curious results obtained at the Waterville arsenal in the great testing machine were also cited. The attempt was made to burst a cast iron cylinder by forcing into it, through a three-sixteenth of an inch hole, paraffine and tallow. But it was found that both these substances became, under high pressures, more rigid than steel and could not be driven through the hole.

Prof. Britton asked Prof. Woodward if the amount of heat radiated per annum could be quantitatively expressed, and in reply Prof. Woodward said it is computed from very meagre data to be enough to melt a layer of ice 5 to 7 mm. thick over the earth's surface. The chairman, Prof. R. P. Whitfield, in closing the discussion called attention to the fact that the submarine crumpling and upheaval were not well known nor often taken into account, and yet they probably far exceed all that we see on the continents.

The discussion will be continued at the meeting of the Section, February 18.

J. F. KEMP, *Recording Secretary.*

SCIENTIFIC JOURNALS.

AMERICAN JOURNAL OF SCIENCE, FEB.

Relation of Gravity to Continental Elevation:
By T. C. MENDENHALL.

Observations upon the Glacial Phenomena of Newfoundland, Labrador and Southern Greenland: By G. F. WRIGHT.

Recurrence of Devonian Fossils in strata of Carboniferous Age: By H. S. WILLIAMS.

Constituents of the Cañon Diablo Meteorite: By O. A. DERBY.

β - Bromvalerianic Acid: By J. G. SPENZER.

The Inner Gorge Terraces of the Upper Ohio and Beaver Rivers: By R. R. HICE.

The Glacial Land-Forms of the Margins of the Alps: By H. R. MILL.

Distribution of the Echinoderms of Northeastern America: By A. E. VERRILL.

Lower Cambrian Rocks in Eastern California: By C. D. WALCOTT.

Pithecanthropus Erectus, Dubois, from Java: By O. C. MARSH. (With Plate II.)

Scientific Intelligence: Chemistry and Physics; Geology and Mineralogy; Botany; Miscellaneous; Obituary.

AMERICAN CHEMICAL JOURNAL, FEB.

Researches on the Complex Inorganic Acids: By WOLCOTT GIBBS.

Diazobenzene Aniline Chloride: By J. H. KASTLE and B. C. KEISER.

On Imido-Ethers of Carbonic Acid: By FELIX LENGFELD and JULIUS STIEGLITZ.

On Some Bromine Derivatives of Paraisobutyl Phenol: By F. B. DAINS and I. R. ROTH-ROCK.

On the Action of Acid Chlorides on the Methyl Ether of Paraisobutyl Phenol: By F. B. DAINS.

The Effect of Hydrolysis Upon Reaction-Velocities: By F. L. KORTRIGHT.

On the Influence of Magnetism on Chemical Action: By F. A. WOLFF, JR.

Reviews; Notes.

THE AUK, JAN.

A Winter Robin Roost in Missouri, and other Ornithological Notes: By O. WIDMANN.

*On the Nesting of Krider's Hawk (*Buteo borealis krideri*) in Minnesota:* By P. B. PEABODY.

*The Nest and Eggs of the Olive Warbler (*Dendroica olivacea*):* By WILLIAM W. PRICE.

A Contribution to the Life History of Porzana cinereiceps Lawrence, with Critical Notes on Some of its Allies: By CHARLES W. RICHMOND.

The Terns of Muskeget Island, Massachusetts: By GEORGE H. MACKAY.

A Swallow Roost at Waterville, Maine: By ABY F. C. BATES.

A New Species of Thryothorus from the Pacific Coast: By A. W. ANTHONY.

A New Subspecies of Harporhynchus from Lower California: By A. W. ANTHONY.

*The LeConte Thrasher (*Harporhynchus lecontei*):* By C. HART MERRIAM. (Plate 1.)

Twelfth Congress of the American Ornithologists' Union: By JOHN H. SAGE.

Recent Literature; General Notes; Correspondence; Notes and News.

PSYCHE, FEB.

Rehabilitation of Podisma Latreille: S. H. SCUDDER.

Two new Species of Entomobrya (Illustrated): F. L. HARVEY.

The Tipulid genera Bittacomorpha and Pedicia (Illustrated): F. M. ALDRICH.

Gall of Eurytoma sp. on the Cat's-claw Thorn: C. H. TYLER TOWNSEND.

Entomological Notes.

NEW BOOKS.

North American Fauna, No. 8. C. HART MERRIAM. Washington, Government Printing Office. 1895. Pp. 258.

Elements of Psychology. JAMES H. HYSLOP. New York, Columbia College. 1895. Pp. 131. \$1.00.

Lens Work for Amateurs. HENRY ORFORD. New York, Macmillan & Co. 12mo; 80 cts.

Proceedings and Addresses of the Second Annual Conference of the Health Officers in Michigan Held at the State Laboratory of Hygiene, Ann Arbor, Michigan. Lansing, Mich. 1894. Pp. 63.